

INTERVIEW WITH DR. FRITZ SPEER  
INTERVIEWED BY STEPHEN P. WARING  
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1. WARING: I first would like to ask you how you got in aerospace research. What led you into space work?

2. SPEER: I started with the war in Germany, I was drafted into the German Air Force and without knowing much about it, commandeered to Peenemunde to participate in the flight evaluation of some of the missiles that were being developed at the time. That brought me in contact with von Braun. I hadn't heard of that time a lot about him, but I was fascinated by rocket developments. This idea kept going in my mind. When I completed my physics study at that time in college, taking at the University of Berlin, I decided that I give it a try. So I wrote a letter to von Braun and told him that I would still be interesting in pursuing rocket development as a professional career. He responded very positively. Of course this was in 1955, we were still technically enemies with the United States. It took a long time to push through all the loops of interviews and background investigations that were required to let people come into the United States. But it worked out that way and we moved from Berlin to Huntsville in 1955.

Then I started pretty much with what I had done in Peenemunde, which was flight evaluation of the Redstone Missile at that time. The project that everyone was working on here and you were part of the Army. For another five years, in fact, before it became NASA in 1960. During that time, I came here to build up a Center-wide evaluation activity.

3. WARING: Was that through telemetry.

4. SPEER: Both with telemetry and tracking devices to find out as much as we can about the trajectory of the missile, how it behaved. In particular to discover anything that went

wrong, even if it was only an anomaly, not leading to catastrophic criteria. These things were very important to know because in the technical development we had to find out weaknesses of the system. For me it was kind of a crash course in rocketry, because almost overnight I became familiar with just about every system in the rocket.

I also got to know many of the people in the laboratories. Laboratories were at that time and for many years to come, the strong back of the organization.

Well, that is in answer to your question how I started in aerospace. Following flight evaluation, I guess I really continued for several years into NASA, before I started doing something else.

5. WARING: I am curious about this period, you were at Peenemunde. Then you were away from the team while they moved to the States. When you got here in 1955 were there big differences in the sophistication of technology?

6. SPEER: I over simplified that a little bit. Peenemunde was not a monolithic organization. It consisted of two parts. I was in the Air Force part. The von Braun and his team had the Army side. Both were highly classified activities. We had something that you called, "Inherent need to know." So, although I got to know guys like professionally Wernher von Braun. I knew almost none of the people later on. These were all people that I saw for the first time when I came to Huntsville. So, I can not make that comparison. Also my perspective is most quite different. During the wartime, I was just a high school graduate. When I came to Huntsville, I had a Ph.D in physics. So in between I had my college education. So I still felt very strongly that I wanted that to be my professional life. I had hoped for, which actually happened, to participate in a lunar landing. I didn't know that it would take that particular shape, but I had hoped that something like that would happen.

7. WARING: What sort of responsibilities did you have during the Saturn years at the Center?

8. SPEER: My first responsibility, let's say there were really two time phases. Initially, I put in charge of the flight evaluation activity of the Saturn vehicles. As you probably know that was not limited to our Center. The launch really was our Center, but then there were payloads put on top of it and other NASA centers became responsible for that.

So, first of all I was asked to establish an organization that was not a laboratory, but sort of a working group that consisted of representatives from each lab. It had sort of a committee-type structure. So I was put in a position to handle all aspects of the flight evaluation from a central point. I was still part of the Ballistic Laboratory under Herman Giesler, but my activities became somewhat independent because I worked with all the other labs. This was very revealing to me, but it also had some issues connected with it. The laboratories were little entities in their own right. They were very concerned about their respective laboratories and they wanted to grow. So my boss Herr Giesler, on one hand was pleased about this additional funds, but then he saw it moving a little bit away from his own responsibilities. So it was a little bit of an issue there.

To come back beyond the internal matter for the evaluation of all flight activities, I build it up in three other centers, at Houston and at Kennedy. This was called a panel. We had several panels that were somehow instituted at Headquarters under Sam Philips and one of Kendall's boys on flight evaluation[77]. They meant to put together the complete story from launch to recovery. Out of this flow and activity, which is somewhat related, the real-time evaluation and the flight control aspects, some of the decisions during a manned-flight could be made during the flight, which is what we call "real-time". So you looked at a place on a CRT and you saw how the trajectory compared with the predicted one. You saw pressures, temperatures and other indicators that gave you a good picture of the [86] for instance because you were always, during Apollo, the S-IVB stage, the third stage, had two

guards. There wasn't an awful lot you could do during the power flight into orbit, but then you had a couple of orbits time to access how well is the third stage and can it be committed to a second burn. During that time, since the third stage was Marshall's responsibility, we had a lot of activities. In fact, I proposed and got approved what is now called the HOSC, the Huntsville Operations Support Center. That was established for that purpose. We ran into a little bit of opposition with Houston and Kennedy. They wanted operation site for themselves, but they couldn't really argue the logic that the design engineers had to be involved with some of the tricky questions. We couldn't always send everybody to the Cape or everybody to Houston.

So that led to establishing a Missions Operation Office. I became the manager of the Missions Operations Offices on the senior level as the other program offices. I had a facility built up of the Huntsville Operations Support Center. I actually had an office in Houston to go perform with the flight control activities under Kris Kraft and John Hodge. That office until recently, developed a rare and good relationship with Houston. I believe there was a lot of rivalry at that time at the higher levels. I talked with Eli Resiboth [106] not too long ago, that it would be nice, he always felt that that was one of the most significant things that I did, at least in this particular area, accomplished a good cooperative spirit within the agency. That is because we didn't really plan to get full-fledged into Houston into the flight control there. We were just support.

9. WARING: So that was essentially the division of labor. You were essentially advisors to the Houston people during the launch.

10. SPEER: Well, it was a little bit more than that. They had the control activities by stage. There was one group that was concerned about the launch vehicle. Another group [was] concerned about the lunar module, still another the Apollo module and so on and so on. They did ask for Marshall people to staff the launch vehicle group. So, these people

were assigned to me. I had about a ten men office at Houston. In addition, we had the Huntsville center to support with advice and support that group and other groups at the Kennedy Center to assist if there was any problem. Of course, if the problem did not commit any amount of significant time, they had to move on. They had to make the real-time decision, we never competed with that. But sometimes during the loading of propellants, you get an erratic measurement and the question at Kennedy is can we go ahead without that particular measurement. That is a question that they would very much like to rely on the engineers that designed the system. So they came back to us and usually we had a half-an-hour to answer this or something like that.

11. WARING: Now was this system essentially set up, namely for the for the Saturn V launches, or was the same system used for the earlier Saturn I and Saturn IB launches?

12. SPEER: We started with Saturn IB, but it really was primarily designed for Saturn V. We knew, the other [130] launch we knew. That is why, I think, that everybody agreed this would make sense. So, cycle one was the first one that I believe came into play. We did exercise the system and we dealt with the facility before then. But I would say that is the primary cause for which it was built. You can say the same about Saturn I and Saturn IB. They were built essentially as stepping stones to Saturn V. Not really big in their own right.

13. WARING: Did you continue on in the missions-operations office up to the time you entered into HEAO?

14. SPEER: Yes. I continued that through the entire flight series of Apollo. Apollo XVII was the last one. Then we started getting prepared for Skylab. [141] on Skylab, but without me. I then was given the job to become manager of HEAO. I think it was in 1971.

15. WARING: Why were assigned to HEAO? Did you ask for the job?

16. SPEER: No, I didn't.

17. WARING: Because your background was in flight evaluation, not in project management.

18. SPEER: [150] and people got to know me as somebody who was interested in space science. I guess I had worked now for 55 through 71, sixteen years in a situation and environment that gave me more and more responsibility. At the time I think I had several possibilities and this attracted me. HEAO was a Phase B study and when it became a big project, they later cut down on it, but initially it was a really quite large project. It would have flown on the shuttle like the space telescope. I thought that was really something I would like to do. I don't believe that there was any application process. They didn't ask for an expression of interest. I don't even remember, frankly, who came to me for the first time. It may have been Eberhard Rees, he was the Center Director at that time. He called me and did ask me whether I felt up to that job. I said, "Yes," But I can't recall in detail how I first started there.

19. WARING: Do you think they were looking for somebody with a space science background and that eliminated certain types of project managers?

20. SPEER: What happens in a situation like that is that the Marshall management knew that project pretty well. We had other projects before. It was not the first space science project. But I think they put most emphasis on the managerial demands and requirements. So they look at the job and then they look at the people who, in their perception and their opinion had reached the level to carry that. The process is not that straight-forward. For

instance, Center Director, in this case Rees, had to go to NASA Headquarters and said, "I propose so and so for this job." Then NASA Headquarters had to vote. Not that they could pick one they liked best, but they certainly can veto. So no Center Director would like to get vetoed on his choice, so there may have been some negotiation that I am not aware of.

I do know that I had very little problem to get accepted. I had kind of a flying start. I knew most people in Headquarters and Marshall were pleased that I was elected and so was I. So I really felt that they had made a good choice.

21. WARING: Could you tell me what you think were some of the lessons from the Saturn program in regards to management that carried over into the HEAO project?

22. SPEER: I think that by far the most important, single factor for me was the experience to deal with different people, their different interests, their different variations. For instance I mentioned the Houston Center which was in some respect almost a new territory because there was fierce competition between the two centers. They always wanted to be in the spotlight, be the first to be mentioned and so on. In the flight evaluation in missions operations, we know that I had brought for so many years, that was a pretty important part. I became sensitive to how to present certain findings, even some like failure of an engine could destroy somebody. So you had to be very careful how you put that. Stick to the facts and don't draw in problems that you have to. I think that is something that I had not learned in school, that I had to learn in real life, and I did. I think that was probably one of the more important things that I picked up during the Apollo time. Working with different environments with different people and even scientists during that time we had some experiments conducted on probability. I was always very interested in space science, so I really liked that idea to include scientists and that type of people I had to work with.

Other than that, of course, I had pretty much by definition to pick up a pretty good

knowledge of the whole space systems, which helped me in HEAO for the space tripod.

23. WARING: One of the unique things about Marshall's management during the Saturn Era, was the extensive reliance on laboratories, the in-house capability and then this very massive penetration of contractors. Was a similar system to that Saturn management system applied to HEAO? Were you self-conscious in trying to draw on that tradition?

24. SPEER: We had to make some breaks with that tradition with HEAO. Marshall had a reputation in a experience of doing a lot of testing, which was expensive. The justification was often that these were manned systems and we had to be absolutely sure that there was no risk for the astronauts and before we could launch. Some of this went, perhaps, even a little bit too far. For instance, when you built a new piece of hardware, he always insisted on first building a prototype that was never to be flown, just to see how things brought out. Then based on the test experience with this prototype you built a flight-unit. Which made a lot of sense for a thousand or more vehicles for the Saturn class, because there was so much at stake. For a scientific payload, where money became suddenly the biggest criteria, even bigger than success, perhaps, to stay within your budget. You had to rethink some of these Marshall traditions. One of the first campaigns I took was to persuade my lab directors and my Center Director to give up on this prototype concept. To say, let's give up on that one year that we would have to build that prototype in and test it before we can build a like unit and simply test the flight unit. Which we didn't pioneer that. Other centers like Goddard had already done that. We had studied in my office how this had worked out. We had very good indications that we could that without giving up too much risk, he felt that if you go from a manned vehicle that is in the limelight of the public, to an unmanned scientific satellite, there ought to be some differences made in order to bring the price tag down. I had to explain why that was a problem was a problem to us. I had only worked about eight months or so on HEAO when it was suddenly killed. [254] he was administrator at the

time. I could hardly believe it! Well, Rees called me into office. I say my first reaction was that I didn't believe it because it had just been put into operation, everything had started, money was spent, contractor had staffed up and all that. The second reaction was, "What can we do about it?" I had very good connections to Headquarters, which was very essential. That is one thing that later was unfortunately pretty much neglected under Lucas. But at that time we had really good connections to Headquarters and we just got a few key people together with Headquarters and we decided that we would try to change the Director's mind. We restructured the program. It took a couple of months. We went back to them. I made a presentation to the Director, he bought it. He said, "Okay, I consider HEAO suspended, not cancelled. Next year [if] the budget looks do-able, you start and you will restart your program." Now this became a much more of a fulcrum instead of launching on the Shuttle, we planned to go on the Titan. We scaled it down to a CENTAUR launch, seven thousand pounds (or somewhere in that vicinity) and the only concession to the scientist was, that instead of two missions, which was the original, two Titan III missions, we have now three CENTAUR missions which was 20,000 total instead of 40,000 total. So it was about cut in half. That was for the funding and we carried more than half of the experiments, but most of the PI [Principal Investigators] had to resign themselves to the fact that they were left alone and [268], but we still had their support. And again, something that I felt very good about. A group of the PI's...you mentioned...each group had a mission scientists. Tom was one and Fred McDonald was one, Steve Hood was the third one. They kind of focused the attention of the PI's on what was going on. They wrote letters to their Congress people and their Senators. I think that we made an impact on [275] who also very interested in Space Science. So I think that it was really this group of Headquarters peoples [sic] and ourselves that turned that decision around.

But for a while it wasn't clear. So for a while I was hanging in limbo. So it was not a very comfortable...

25. WARING: So it was after the cutbacks that there was a decision to go with the protoflight concept?

26. SPEER: Yes, I think happened in the same time period. After the [283] question came out, "now how much money can we put aside for the experiments and how much money will have to spent on the spacecraft contractor, who I believe was TRW. Of course TRW had started out as a big contract and we had to cut them down, which is a very difficult process for the government. If you don't have competition between several companies, you deal with one, he has you in a pocket. But I think that TRW reacted very responsibly. They knew the program was at risk and they made a very major contribution to save the program. It was still very expensive in a way. Had we started out as a smaller program, we could have had it for less money. But the money was spent on getting ready for the big program.

Somebody asked me later on since HEAO came out within cost and essentially on schedule, would you recommend this kind of approach for a future science project? I said, "Hell, no! That is not the way to do it. It is expensive and it risks everybody." We all wanted to do it, but I would have not been surprised if some people would have jumped off and said, "Well, if you want to cancel it, please go ahead and cancel it."

27. WARING: Was there consideration on the part of Marshall management that they really had to go ahead with HEAO to save jobs for Marshall personnel?

28. SPEER: That was always a consideration after Apollo. It was very clear that there were certain people at Headquarters that were seriously thinking in terms of do we need all NASA's centers without an Apollo Program. I am sure that was a consideration. But I think that it was even more direct. I had very much the support of all the Marshall

management to do this restructuring. We even got additional people assigned temporarily. So this was a good time, everybody brought together, overtime, weekends and it was very intense. But everybody enjoyed it. I think that it was probably more the feeling from Marshall management that they wanted to have this science project, than to save the 150 people or whatever it was, that ultimately worked on HEAO. We wanted to expand into industry science and as you know that was really the pioneering step. By then we had space telescope and the Skylab and all...well, Skylab was not at the same time. ATM was actually before HEAO, and you can see that was what our strategy was. We had to get away from the connotation that Marshall was just for launch vehicles, nothing but launch vehicles. That was a very important part of the strategy. HEAO became more important to Marshall management then justified by the numbers. It was a new thing. It was in a way territory that other centers had occupied before. HEAO would have been a typical project for them. But at the time it was a big science, bigger science project, three missions in all. Goddard happened to be pretty busy so they didn't make a lot of fuss. They accepted that Marshall would occasionally have these bigger and very complex science missions.

But these are the main reasons that Marshall felt strongly about and was willing to make compromises like giving up on the prototype. We did want to establish a good relationship with the scientists and they were very critical about putting too much money in the spacecraft and too little money in the experiments. They wanted to proper balance. If you have a spacecraft prototype, you really are taken away from the experiments. We showed that in some charts and I think that really convinced everybody and the fact that other centers had already done it successfully.

29. WARING: What was the division of labor between Marshall and Goddard? Was Goddard active in helping to put the instrument packages together?

30. SPEER: No. That was one big advantage that we had without really knowing it. The

whole project was in our hands, from A to Z. We had the design, we started with the definition effort, then the design, development, the launch, the flight operations. We did conduct it out of Goddard, similar to Apollo there. I had had an office in Houston, I now had an office at Goddard. They helped us, but under Marshall direction. We had a flight director at Goddard that was a Marshall employee.

31. WARING: So once the spacecraft was up and operational, then everything was turned over to Goddard?

32. SPEER: No, we still had full responsibility. See, it was a time-limited mission. Our mission requirement was six months for the first in the set, and twelve months for the second of the C-missions. We did about twice as good in all three of them. But it was time limited. After one year, we went into the second one and the first one had just about expired. The second one lasted longer than the year. Then the third mission was launched. Why we had two missions at the same time, but then the HEAO-2 came down and three entered....all of that was supported by Goddard, but by Marshall management. It was very unique thing for us. We, at Marshall, were used to turning operations over to another center. But in HEAO we had full responsibility for ever.

33. WARING: I didn't realize that. I don't think that is explained in Wallace Tucker's [The Star Splitters] book.

34. SPEER: I don't remember.

35. WARING: Would you say that is really amongst the first operations responsibility that Marshall had?

36. SPEER: Yes, it was. You have to count also, our operations responsibilities on Apollo and Skylab. We had some. It was still under somebody else's direction, but we had some operations responsibilities, even there. On Skylab, of course, we had the meteoroid shield come off during launch. Very important activity was conducted at the HOSC. So you know, you can't say that Marshall never had a voice in responsibility before. But as a general rule, at least on the HEAO class of spacecraft, they did want to keep the package together, which now, I think, everyone realizes that was a very important decision. Then when things grew to still a different order of magnitude on the Space Telescope, it was thought we can't give it to one center, so Goddard became responsible for the instruments as well as for the operations. But that created problems. But now I think that the pendulum is swinging the other way again. With AXAF, as far as I know, everything will be in Marshall's hands.

37. WARING: Do you have a lead center...?

38. SPEER: Including the [401]. It removes a lot of potential problems. On the Space Station we don't have that and some critics outside NASA see that as a potential problem. Splitting certain separation claims, splitting the responsibilities between several centers, that are known not to work ideally together.

39. WARING: How much in-house development was there on the HEAO project? Was there much in the way of instrumentation or technology that was developed in-house?

40. SPEER: No.

41. WARING: It was all TRW's activities?

42. SPEER: The spacecraft was a TRW design. It was very carefully reviewed by a lot of Marshall people. It was a little difficult for TRW. They were not used to the kind of penetration that we talked [about] earlier. We really penetrated TRW and they weren't use to that. I had a pretty difficult selling job to convince them that was really for the good of the project. But, on the other hand, there was no in-house design. We had two proposals for HEAO, one from Grumman and one from TRW. TRW proposal was a very good one. We did direct them to accommodate a few features that we had developed in-house. But basically it was an TRW design.

The instrumentation was given to Principal Investigators, in a response to an announce of opportunity, they would submit a proposal to NASA. Some of them were accepted and some rejected. On HEAO we had a much large family of P.I.s and due to the physically limitations of the smaller spacecraft, that number of P.I.s had to be shrunk. What happened was that we did give some of the other P.I.s, at least some of them, a chance to participate as co-P.I.s. So they lost their experiment, but they didn't lose participation.

43. WARING: They still had access to the project.

44. SPEER: Access to the data and they even had some say in the designing of the instrument. Each instrument was really a brain-child of that investigator.

45. WARING: What was the role of the labs during HEAO? Were they primarily supporting the project office in the normal way or were the labs helping to develop ideas with the P.I.s?

46. SPEER: The labs had an automatic responsibility for all technical reviews. We go to a design, we start with a requirements review, with a preliminary design review and then with

a final critical review. So there are three major reviews. The laboratories send their best people to these reviews to get the information from the contractors prior to the review and then ask questions. Then they issue at NASA, what Marshall terms "RID" [Review Item Discrepancy]. These RIDs represent their input to the project. We had to take it very seriously. We had to disposition every one of them. Some of them where there was misunderstanding from the part of the lab because they had not participated in the design. But some of them were very valid challenges to the design and actually lead to design changes. So it was a very active and good process. This was one activity that the labs helped with.

The second one was that they conducted some tests. But on HEAO, because we were in such a constrained financial situation, you really didn't have a system of reviewing activity at our prime contractor. We decided to do that in-house. So in addition these review activities, there were a group of people who were my systems engineers and made sure that everything out there was hanging together. The minutes, the change control system for the project, also conducted some very specialized tests that, that for financial reasons or for reasons of not being available at the prime contractor, we decided to do in-house.

So it [the labs] was reviewing, system engineering and it is testing. Of course, reviewing was a almost continuous activity that included trips to the contractors. I am including not only TRW, but also all the experiments. In many cases, these were very important too, and had design problems. Some of them even asked for help. So of them were not so open to asking for help and they didn't particularly enjoy being x-rayed on their design project. But all in all, I think that we had a good working relationship between the labs. A kind of mutual respect between the key people on the part of the contractors and the key people in the labs.

47. WARING: Wallace Tucker in the Star Splitters, quotes several of the PIs as having

been uncomfortable with the multiple reviews and the intense scrutiny by Marshall. What is your reaction to that? Do you think that is just natural on the part of scientists to do that?

48. SPEER: Yes, I think it is, natural. These people some of them built the instruments in-house at the university to apply government standards that were developed over many years for industrial contractors. This was probably not a very smart idea. We tried to defuse that to some extent. But still, the PI felt that he was in control of his experiment and he knew better than anyone in the world what it should do and how it should be built. He minded somebody from Marshall who he considered not on par with his scientific capabilities to start questioning him on some things. So, there was a little bit of friction. But I think we overcame that with the help of the Project Scientist and also with some really good people in the laboratories that gained their respect. So, although there was some criticism, I believe all in all they accepted it and finally were pretty happy when we had this HR [733] problem, for instance. I think that it is a tribute that we have a group at Marshall that were very familiar with [736] and presented a good counter-balance to what Bendix was telling us. We always had a valid voice in these discussions and I think that if you would ask these PIs after the project was completed and the mission was successfully done, I think that they would say, "Yeah, we didn't particularly like it, but we agree now that it probably was not a bad idea to go through this sort of scrutiny." But that is my perception. There may be some exception to the rule but I think that would reflect the majority.

49. WARING: It was a very successful program.

50. SPEER: It was a successful program and of course, success in hind-sight justifies probably the means. They certainly didn't like it when it happened. In some cases, I was

the project manager, but I could not always be there when our people had meetings with all the PIs. That happened practically everyday. I am sure there were occasionally harsh words on both sides. When it hit a certain noise level then I had to step in, but normally I didn't have to step in. From my perspective, I think that it worked out alright. But TRW nor the PIs were not used to scrutiny. Other NASA centers who had flown similar missions earlier, HEAO and all these others did not go to that degree of penetration as Marshall did. It was our heritage and that is how we did business. Even if I could, I probably would not have changed it. After going through the Apollo experience that that was a basically good system with some adjustment, but not a complete drastic change.

51. WARING: In what ways do you think the HEAO project was significant in the evolution of the Marshall Center? What was the meaning of HEAO for Marshall?

52. SPEER: It opened the door to a new dimension of our business. It got a lot of people in contact with science ideas, science concerns. Since we are now a multi-project center, Marshall recognized as such, recognized as one of the versatile centers in the NASA family, I would say it was an extremely close step to get into that part of the business. I believe it was very definitely a very major point in our favor to be selected for space telescope and AXAF that we had done HEAO so well. Suddenly our Headquarters, our main partner at Headquarters was, of course, the Office of Manned Space Flight, which was in charge for all the launch vehicles. But here, suddenly, we were offered business with the Office of Space Science. At one time it was called the Space Science and Applications, but that later on became split into two, we had dealings with the Office of Space Science. I think that HEAO established Marshall as a main player for OSS business. They used to spend all of their money with Ames and CPR and Goddard. Suddenly, I think that we became a major worker for them.

53. WARING: Did you ever feel while you were working on the HEAO project that Marshall management, or perhaps people in the laboratories felt that this was a secondary program to the development of the Shuttle and propulsion systems?

54. SPEER: Yes and I excepted that. I was part of Marshall management at that time. I was reporting to the Center Director and if you had asked me I would have say, "Our authority is too develop the successful...."

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55. SPEER: ...Space Science, slightly different rules would apply. But in an absolute prioritizing I would say that there was no question that Marshall's major priority was Shuttle. You could tell in terms of money, you could tell in terms of people assigned to the Shuttle versus HEAO. By every standard we did not carry that authority. It was not that important to NASA that HEAO succeeded, but it was very important to Marshall to become a member of the Space Science club. NASA's whole existence would depend on the success of the Shuttle. Now if you ask the same question in regard to Hubble and to AXAF, I think that it is still true that getting ready to fly with the Shuttle again, and the space station carrying high priorities.

But I think that is really true for most peer centers, that you have a hierarchy of priorities. Depending on value and money, the question whether it is manned or unmanned, the lines on NASA for this particular project for the future of NASA as opposed to addressing only a small constituency. I certainly accepted that without any problem.

But, on the other hand, the numbers of people that worked for me were smaller. But I never felt that I had less priority, because some of the challenges we had were very demanding in [839] sense. When you have a very demanding and challenging problem, you

do get the best people. We had then developed this matrix organization, where each lab would support all the programs. We introduced a pretty stiff and rigid system of task assignments. But, everybody was very happy to break the rules if there was suddenly a program and they had to step into the support. I mentioned the rainjowls [845], we had a few things...the motor on the second HEAO, the HEAO-B was extremely difficult job and I think was our first challenge in terms of astronomical know-how. We had so much problem with on HST, but I think that this was our first experience and we definitely got feared. We got some very good people assigned to us. My point is, while number-wise and money-wise we were certainly advised more in comparison to the biggies, the quality of the people was very good.

56. WARING: Do you remember in the midst of the HEAO project making requests of Marshall management that they try to hire more Ph.D astronomers and physicists? Was there a definite concerted effort to "beef-up" Marshall's space science personnel, given that they were moving in new directions?

57. SPEER: Yes. Our Space Science activity was always oriented towards the Marshall assigned projects. So in Skylab we were interested in solar physicists. On HEAO we were interested in astrophysicists, not very successful. We didn't really have too much to offer. But Marshall management knew in the long-run you can only succeed if you have the scientific equivalent of the engineering force. So with HST, that was suddenly a very strong, high, commission that we had to have a top-notch astronomer. Gallup [863] was hired for that reason. I guess we still are not that well known in the scientific community as a science center, but the efforts are still being made to at least have the project scientist, like Marty Wieskopf [867] ...on the space telescope to be a part of our center. I think that effort, of course I left a few years ago, so I don't know what the feeling is now, but I assume that Jack Lee would want to fill in whenever there is a vacancy of resource scientist,

especially at home and future science projects [872], like AXAF and space station, microgravity research...that type of thing. I think that they are very respectable science activity, but one that is not in competition with science. That is one mistake that I think Goddard made, when...

58. WARING: They set themselves up like a university?

59. SPEER: Yes and they were very keenly aware of the problem I just tried to explain. They probably went a little overboard. It happened very suddenly before they knew it and very strong science complement and these people were very aggressive and successful in gaining approval for carrying their own experiments into flight. Suddenly the outside community realized that there was a very bad sort of incest, that NASA decided to fly science missions and here are NASA scientists who win the competition of flying their experiments. It had to be very, very carefully with outside peer group abuse, it had to be very carefully orchestrated to get away from that accusation. I think there is still that feeling in the scientific community that NASA wasn't quite fair. They gave a little bit of advantage to their in-house scientists. So that is a danger and I think that Marshall was clever in avoiding that pitfall. But we probably are not quite up to the science competence that we should have in these missions under Marshall management. It would be good to have a few more astrophysicists and a few more astronomers.

60. WARING: Do you think that was problem that developed in during the Hubble Program, or was that just a relatively minor issue besides the huge issue of funding?

61. SPEER: I don't understand your question.

62. WARING: Do you think that Marshall's management of the Hubble Program suffered

because it did not have enough highly skilled astrophysicists?

63. SPEER: No, I don't believe that was a critical issue. The fact we had Bob O'Dell here and he had some of this students and his work with other students going on, was properly sufficient from that point-of-view. I don't think that adding two or three astronomers would have helped with this problem because we were in very close contact with all the PIs, regardless of where they were sitting. Some were at Goddard, some of them in universities. We had a science working group that was shared with O'Dell and interacted with the project and all the important questions. So I would not feel that led to any to problem or could have made a difference.

64. WARING: Marshall was able to rely on other ways of getting scientific advise so that in-house scientists would not have made much difference.

65. SPEER: As it turns out, the science things are working fine. The only thing that did not work out fine are in the mirrors [910]. There of course, we had a very peculiar situation, if you want to go to the space telescope now. When I inherited the job after HEAO was done, Bill Keithly [913] was the HST manager at the time. He had an offer to become the program director at Goddard and wanted to do that, which I think put Lucas in a very difficult position. But, I was available and I had done HEAO successfully, so it was logical. Again, he had to go through all the loops at Headquarters, and to my knowledge, everybody agreed that would be a good choice. But I really, in hindsight, would say I almost feel I was set-up because I inherited a system that I was totally unable to change; it carried the signature of another man. And although I was able to change some of it in the long-run, initially, it was just carried forward exactly the policy and the choice that had been made. Since I now know that Perkin-Elmer was not the only bidder for the optic system, that Kodak was the other bidder. That Kodak has proposed the important optical

systems test and Perkin-Elmer felt it was not necessary...all these things were never explained to me so I never questioned it.

66. WARING: Why did you never know that?

67. SPEER: I guess the main reason was that we had so many urgent problems shortly after I arrived at HST, that there was hardly any time to go fishing for any potential problems because we had enough real problems. Most of them were money. When I arrived within a month or so, there was a major review for HST in terms of [whether] its funding was sufficient and it was entirely intolerant [936] to NASA and mostly intolerant to Marshall. I think that there was at least one Headquarters representative, but that was about it. It clearly showed that we would have to have extraordinary luck to make it. But Norman took note of that. I guess we were all growing up in an environment where we knew that Congress would only give you so much and you had to build up the momentum of a project to carry it through. If you have an overrun later on, you will get more money. But if you declare at this moment that you might have a potential problem, you may really risk the project being canceled. That is how everybody continued to build up the momentum. Many people, including me, knew that eventually we would need more money then was set aside. But we never said that to anyone outside of NASA.

The problem that I was going to talk about was that...I guess that it is still not completely clear that you can talk about it totally openly, but there were severe restrictions as to the degree of penetration. We talked about penetration in the nearing sense earlier [950]. Here were approaching a technology that was on the cutting edge of technology, which was also of interest to agency outside NASA, in particular, defense. We accepted a contract with Perkin-Elmer, who had apparently a lot of experience in the black world with classified projects. We accepted that. By "we," I can't include myself. This was all happening before I joined. But Lucas and the key people in the project, were aware that

we were borrowing technology that had already been used for other purposes. The contractor that we had chosen had every success and thirdly there was severe restrictions to the amount of technical penetration that we were allowed to apply to these technologies, simply to avoid transferring this in an uncontrolled way...a very understandable thing. But it did mean a major departure from NASA's normal way of doing business. I think, I don't know the exact number, but I seem to recall something like ninety to a hundred people was the absolute limit that was imposed on Marshall to put on this project.

I believe that there was another consideration as well. Marshall was had the reputation to do everything in a grandiose way...in a way that would cost money. We had a good reputation for being successful and not having any failures, but at a pretty stiff price tag. So some of the people in Headquarters, and since I did not directly participate, I will not mention names and most of them have left now, so you don't need them anyway, felt that, well if Marshall was to get this project, they would have to promise that they would give more freedom to the contractors. In this case, Lockheed and Perkin-Elmer. [And] that we would put more confidence in their ability to do the job without the penetration that we were use to. So it was the concern of our technology transfer and it was the concern about the carelent [985] way of doing contractor penetration. I guess a little bit more than that it could be political in that some of the bigger firms like Lockheed did not like being overseen in that sense. They began to have some political muscle and all of these things came together to...like Kingsley, and Kingsley accepted it rather freely...to put only a minimum of Marshall participation into that project. We had a few very good people, I don't want to belittle that, but on a complex program of the magnitude of Hubble, you just need almost a comparable number to Apollo, to really look at everything in depth and to stand up and say, "Yes, this will work." In particular, I think that we were somewhat weak in the mirror arena as everybody knows. We really didn't have anybody that could tell Perkin-Elmer how to do this. We had some telescope scientists, who were astronomers, and probably the most likely ones to find anything wrong. But they faced the same

problem of finding locked doors. They could not always be there when important tests were conducted. Perkin-Elmer pretty much had their own way of doing it and they didn't want us to look into their costs.

68. WARING: It is almost like the contractor was in charge rather than ...

69. SPEER: Then the other problem that we had was that there was a system of two associate contractors. No one was really prime. There was no prime contractor. They had two contractors that had an unusual freedom to act on their own.

70. WARING: Perkin-Elmer and Lockheed?

71. SPEER: No one was prime, so no one could tell the other one what to do differently. Marshall did not have sufficient penetration, sufficient involvement in the program to really lead it the way we were use to. That was the thing that I began to change. I did have good help from Lucas, who sensed also that this was not right. In the meantime, the concern about technology transfer had been slightly reduced. Although it was still some concern. But, we were able to double or triple the number of Marshall involvement over the three years that I was the head of the project.

But of course, the money caught up with us and it was quite clear that within a relatively short time the [019] very major infusion of money would have to be made. This in turn meant for NASA that they would have to confess to Congress because NASA had made a very strong commitment in earlier years there would be a cap on the project and this cap was in jeopardy. I think that is one thing that is probably very unwise, you have a choice of two things. You can consider the requirements negotiable or you can consider the cost negotiable. But to put yourself in a box on both these things is a very dangerous move and I don't know why this was done. We did make an attempt to reduce the

requirements in an effort to solve the money problem. In other words, descope. O'Dell ran into a real fire storm when he proposed to not fly one of the experiments, because that would really saved some money, if he had said, "Well, we are a telescope that will be revisited with the Shuttle. It will be refurbished anyway, why not take advantage of that and solve our money problems by delaying one instrument, the one that costs the most, is least important or whatever." He even proposed the particular one, I believe. But the science people felt betrayed. They felt that we would fight to save our skin on their backs, so it created a break in mutual confidence and mutual trust. It was a very bad thing. Headquarters, it turned out, didn't want any part of it. So, it comes back to this dilemma that if you are unwilling to change your requirements and if the technical challenges are such that the technology doesn't exist and has to be developed, then you have to add more money. People were unwilling to pave the way for that. NASA was a little bit short-sighted. We certainly informed them of the situation, I think, in a very good way. During the break-point in 1983, a lot of accusations were made from Headquarters that we never informed Headquarters. I think that it can be demonstrated that is a patented lie. They were very well informed. We had occasionally very open discussions with some of the key people and agreed on the overall strategy. I think that there was no question in their mind that we would eventually need more money. I think in hindsight, we could probably have been more clever to play this game with Congress, to go back for more money at an time where we weren't in a severe catastrophic situation. What really made it more critical than anything else, was Perkin-Elmer's inability to really plan ahead. They had given us their estimate to complete. The government required each contractor to do repeatedly these estimates complete. We have found out that Perkin-Elmer had no substance to that, they simply didn't know what it would take to go through all the stages of testing, assembling the mirror system. We had [064] overrun. It was fabulous compared with the original estimate. We finally had to move in a group of Marshall people to really reside there for many, many months to do some of their jobs in planning the various steps that had to be taken to

complete the optical telescope assembly.

Lockheed, also, came out with much more money than they had originally told us. On the other hand, they were in a position to better estimate what it would have taken. I would also have to say that their job was a little bit easier, more straight-forward than the mirror. The mirror then took almost exactly twice as long as we were told. No one at Marshall told me that that wasn't right. We felt that it could be done, how it was based on spin [076].

Again, I am talking about something I didn't participate in, how that estimate was arrived at. But it is very uncomfortable for a Program Manager who carries with him a great deal of responsibility for a project, for [him] to come into a situation which is really pretty much determined already. You have only limited room to maneuver. The fact that these strobing [082] was unacceptable was not known to me. In fact, the money limit, the cap, on the telescope was also not negotiable to Congress, was also not known to me. In fact, I was told otherwise. It was a very sobering experience for me and one I guess I will never completely recover from.

72. WARING: You covered that very well. You have answered almost all the questions I had listed for talking about the space telescope.

One of the things that you mentioned earlier was about the relationship between Goddard and Marshall. Could you talk more about their relationship on the Hubble?

73. SPEER: When I talked about Perkin-Elmer and Lockheed, I left out the out the fact, that the instruments this time, and that is different than HEAO, were not only outside Marshall, they were actually managed by another center. This started out pretty well under Kingsley and Levine. Then when Kingsley moved to Goddard, either he or his boss, decided that they wanted to take better control of the instrument management at Goddard. So some dichotomy developed. It was difficult for me to get first-hand information from

the PIs, because they had to report to another center. Fundamentally and theoretically, I should get everything I wanted. But there was a time-delay. If there was a problem I would not immediately know about it. It would have first to be worked at Goddard. Sometimes I don't even think they told me about it because it was resolved before it became a big problem. So the only thing I saw occasionally was an increase in plumbing [111] requirement. So it simply watched every move. You didn't have a person in your office for your staff meetings. It was another center, it was barrier that really acted as an impedance. I can't say that I would consider this a major reason why HST didn't work out as well as it could have. But it certainly didn't help to have this management split. Comparing it with HEAO, on a different scale of course, my job was a lot easier. I had an experiment manager who was next door to me. We would settle problems right away. Here there was a different geographical location, our conferences due to telecon [124] problems, but mostly on a telecon you couldn't even see their face. Sometimes I went out there, but you couldn't do that every time. So there was that barrier.

When money problems occurred, there was a tendency on Goddard's part, which you would expect, to show that it was their problem, when it was really a problem on the other side of the interface.

74. WARING: So it was like the two centers were competing for, perhaps, the same amount of money?

75. SPEER: Yes, I would go that far like competing. They had to realize that it all came out of the same pocket. But there was a tendency to shift responsibility for problems to the other side. For instance, we didn't get input from the experiment as we were told, "Well, you didn't get the input because some of the basic data they needed before they could develop the input from Lockheed didn't arrive there on time." So one guy played against the other in these terms of excuses. Headquarters tried to help and we had quarterly

reviews, where everybody got into one room. But these were only quarterly reviews. It certainly is true that things could have worked smoother if only one Center had been in charge.

But again, nothing catastrophic happened. I did mention, and I don't know whether you know or not, but there was still another party involved and these were the Europeans. The Europeans were responsible for one of the cameras and the solar array. Of course the solar array and the activating mechanism that kept the solar arrays oriented to the sun are an extremely important part of the telescope system. So there was an additional complication. But here I have to say that this worked beautifully. It was a pure joy to work with the Europeans. They were understanding, pretty than with Goddard. There is no question in my mind. But then we were a whole continent apart and some of the technical interface problems, didn't get visibility. Now, the difficulties that we run into when the solar array gets into the sun or gets into the eclipse, I think, are to some extent due to the fact that we didn't not understand their design completely and they didn't know enough about space. That reflection that these things ...[? 162]..no one did foresee. I don't know, I can't say that for sure. I believe if that system had been developed here by TRW and by Lockheed, probably we wouldn't have that difficulty. It is something that we can deal with and compensate for with the software. Again, I think that it is a manifestation of the difficulties if you have too distant partners working together. It is a lesson for the space station too, and I am not sure that we have learned it.

76. WARING: Do you think it would be a useful generalization on the part of a historian to say that the Space Telescope Program was essentially driven by politics and money, rather than driven by technical and scientific requirements?

77. SPEER: No, I think that would be an over-simplification. You see, the originally requirements were generated by a the science group. They really pretty much didn't

change. What did change was that some of the complications and consequences were not fully assessed. For instance, I took the job in 1980 and at that time, we didn't have requirements for meteoroid contamination [? 181]. We started developing this. We didn't even know what the requirements should look like. Finally, we gave that complex change to Perkin-Elmer to build a mirror-vacuum system. That was never in the contract initially. But these are the things that you do not know. Another thing is that we wanted the telescope to be able to follow moves around the planet. In other words, moving targets. You have a space fixed orientation, but then you want to put into the software a capability to follow an orbiting objective. Again, that was an additional cost that we may never have fully realized when the contract was let, all these were unknown [182] changes.

You said, "political?"

78. WARING: Certain things had to be done to sell the project to get funded.

79. SPEER: The selling of the project was a very difficult process and I think that you will [find] a very good description in this book. I can't really add to it, because I did not witness all the things as people who contributed to this book. But it was a very difficult process and it didn't come easy. So the cap was meant very seriously and I think not enough attention was paid in NASA to that simple point. Certainly not by me, no one told me, "Look, you go into this, you better open your eyes. Here is the money left and here are the uncertain areas." I almost played a game, saying what are the gaps in the program. Almost every month we found a new gap. Every gap we found meant additional money was to be spent. That, and in addition to the fact that some of the contractors, Perkin-Elmer in particular, were not able to really, truly assess what those would cost. We know what would happen. I personally believe that we could have restructured the program at the time, roughly when I came on board, and probably come out much better. Certainly not with this catastrophic overtone that everybody messed up the program.

So political influence was very strong, you are certainly right, but the initial requirement came from science. The fact that the Europeans were invited to participate was a political project in order to reduce the all-over cost, this was suddenly a complication and Congress wisdom only saw the money side which was good, and didn't see the complicated side. We have all paid for that with the initial problems in the fine-guidance center.

The fact that two centers got the job and sort of...well, Marshall had the lead, there was no question about it. But, dealing with another center is a very special environment. You can't tell another center what to do. It tells you what it will do.

80. WARING: Especially when Goddard had the expertise in instrumentation.

81. SPEER: You were asking for data in a certain format in order to fit your system. They said, "We can't do that. We will give it to you in our format." So, there are these things that made the daily life a little more difficult.

82. WARING: So in terms of management lessons from Hubble, the problem: a prime contractor was probably needed; more flexibility about funding.

83. SPEER: System Engineering was played down for financial reasons. You can really put it on a nice, simple denominator: the program was underfunded. You cannot get something like that for the money that was set aside. It also took a lot more time. I think that you pointed out a very important one, there should have been a prime contractor that would have been over the subs. There should have been a very strong Marshall penetration from the beginning like any other program. We should have refused to follow the Headquarters advice on that. They are the ones that complained later. They didn't realize that they had told us almost the opposite from two years ago, earlier.

84. WARING: So perhaps another lesson is there is a real danger to have Headquarters managing a big project when those people lack expertise.

85. SPEER: I think the problem on HST was not so much that Headquarters wanted to do too much. There was an unusual change in leadership. We had within a very short time, you will find that in this book too, I believe it was five different program managers. Every time you got a new guy on board, you have to start over again. There is a lot of loss time and the possibility of misunderstandings and misimplications. They are confronted with the same dilemma that I was. They inherited the system. They had very little to do with ...they couldn't really move very much. They had only limited visibility. They could not attend every meeting. But, I could not say that Headquarters made an effort to manage too much.

After I left, they made a major effort to direct it out of Headquarters. I think that was a mistake. But before that time, I would not consider this a complaint. I would say the program director at Headquarters takes on a job like the HST ought to have to sign off on the dotted line that he commits for a certain number of years and not only for half a year or so. That was ridiculous for something as important as HST. That part even goes for the project manager. I am not sure I agreed that Kingsley should have left at the time he did. He had set the whole thing in motion and he got out when it got hot, because people said, "Well, it can be in your way for your career." He needn't have made a commitment to leave this project, there was no real forcing reason for him to leave it. So I think the turnover was too high to be healthy.

Then to sent us some work that was very unsolved. The European workmen, but lead to some technical difficulties.

86. WARING: The involvement in the Department of Defense and their limitations?

87. SPEER: That was very detrimental, because it prevented our initially penetration, if you want to use that word. Really, recognition of the key problems was initially very, very short. After the we ran into problems, that changed rapidly, but that is the wrong kind of help that you get only in a crisis situation. You are always with the overtone of "who is responsible for this?" rather than "how do we solve the problem?"

So I think, yes, this is a good case history for how not to run a big program.

88. WARING: Rightly or wrongly, a lot of people are already saying that a lot of responsibilities can be traced to Bill Lucas' center management style. That is, the problems with Hubble, the problems with the shuttle, can be traced to Bill Lucas. I realize this is a very sensitive issue, but what is your reaction to that?

89. SPEER: I like Bill as a person, but I don't really believe that he was the right person as the Center Director. He had a habit of running things himself, of not really delegating things to others and being almost paranoid in not working with his superior in a variable compression. He always...a very famous term he used was "gotacha"...he always reminded us if you say this, he will always make you responsible for it. So he created a very unhealthy atmosphere of keeping things for yourself. He did come along with me to some of the sessions with Headquarters and played a fairly useful role. Certainly, I can't complain about those few times. But, all in all, he did not creatively [address] the fervid problem interface with Headquarters. Not only didn't do that, he also encouraged all the people reporting to him not to do that, not to calling it open. Sam Keller complained about the interface with Marshall. I think that he was primarily addressing the interface between Lucas and himself, not necessarily between me and Pollard, because that really worked better. Bill's tenure is, in my opinion, really was a bad period of time for Marshall. He took the fun out of doing space work. Of course, a very clear difference in the way people worked with each other and communicated with each other before he came on board and

after he came on board, was a gradual thing. It didn't happen overnight. After a few years, everybody realized what had happened. We were very concerned about the wrong message getting out. The press couldn't be told anything, Headquarters couldn't be told anything. The other centers shouldn't be told anything. Extremely competitive is how I would characterize that.

At the same time, he was trying extremely hard to do a perfect job. He didn't realize it that with human interface he really failed. He was, in my opinion, a total failure in communicating and treating people. If somebody made a mistake, he was gone, he killed him in a meeting. What a crazy thing to do. Von Braun did the opposite. If somebody made a mistake he would always say, "Next time do that better." It must have been his upbringing, I don't know.

90. WARING: Do you think, to change to a more general subject, do you think that the way that NASA is organized with Headquarters just sort of general oversight, and then the centers in many ways, very independent. Do you think that NASA's way of doing things require successful center directors? Do you think they are very important in the development of....?

91. SPEER: Well, I think there is a choice. We talked about the difficulty of centers working together. If you accept that and you say, "Let senior centers do their job pretty much in total responsibility, then you want a strong capable center director who can really put together the best of his people to form teams and to do an excellent job. One that has a confidence effect for us and will give us enough room to play. The other option that NASA has is to say that we are sick and tired of these little czars and we want people who are team players. Then maybe you may be able to work a cooperative project between centers. If there is enough of a relationship and enough of these projects that go around, then on one is Goddard leads, on another Houston is lead and on still another one

Marshall is lead and all the others fully supported, it might work. I don't know. I personally feel that von Braun was a pretty good example of a strong center director that would be able to motivate people to the best of their capability and would get enough rope from Headquarters to do his job and would have enough intelligence to see that he can't do everything at NASA that there would have to be certain communications. He and Gilruth could work together even though there were center personnel who were pretty ambitious and didn't forgive easily if the other one got a bigger piece of the pie.

I would be inclined to consider this in a more careful approach, because I just can't believe that you could find that many real good team players, that don't put the interests of their center on top of everything else. We joked about having ten NASAs rather than one NASA, each representing a center. But then in a way, if you go back into Marshall's history where each lab had a lot of power and it seemed almost impossible to control these individual interests in a way that would serve the Center. So that is in a small way a comparable issue.

92. WARING: There were eight Marshalls.

93. SPEER: And ultimately these laboratories did get together. Now the way it was done was to remove some very strong personalities that were leading the laboratories and make the newcomers more dependent on the total thing.

94. WARING: They removed a lot of the personnel so that the labs could not do things all on their own.

95. SPEER: Correct. They did a little bit better distribution of the job. That would be the answer for NASA as well. Attempts have been made to give each center only certain focus areas that would not overlap with the same extenders that is now the case. Then Marshall

would be launch vehicles, Houston would be manned spacecraft, [Goddard] unmanned spacecraft. But, everything that we have been doing in the last fifteen years goes in a different direction. To cut that off now would be very dramatic and probably controversial.

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96. SPEER: ...convenient packages that a given center can be fully responsible for and not depend on critically on other centers. Now, Space Station Freedom is, of course again, something that depends on many centers really working together. Learning from past experience they have put a strong Headquarters office and made that the leader. This has, to some extent, backfired. So the activity is moving to some extent back to fear [008]. I think that we haven't heard the last of that yet. I think that is a problem that NASA will have to wrestle with and find a good, ultimate solution. The solution, in my opinion, will be different depending what kind of projects that we are doing. If we go to the new exploration initiative, that President Bush has announced, this is a so much bigger job, that we need definitely to restructure for that. Then there is a time to address this question. If we do not do well each day with relatively small projects, my prediction will be that things will be pretty much left the way they are. We probably avoid getting work at the center in [015] and single projects as long as we can.

97. WARING: Very good.